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Forgetting in Primed Fragment Completion

Steven A. Sloman and C. A. Gordon Hayman
University of Toronto, Toronto, Ontario, Canada

Nobuo Ohta
University of Tsukuba, Tsukuba, Japan

Janine Law and Endel Tulving
University of Toronto

Seven experiments examined the time course of primed fragment-completion performance. A pilot experiment and Experiment 1 showed that rapid forgetting occurs immediately after study for a period of approximately 5 min. The rate of this immediate forgetting is independent of the length of the list. Experiment 2 showed that priming effects were still present after 16 months. Experiments 3 and 4 provided further evidence of forgetting over 1 week. Experiment 5 showed that retention performance after 20 min is unaffected by the interpolated study and recall of other lists of words. Experiment 6 showed that 10-min retention performance was substantially reduced as list length was increased from 10 to 100 words; but it showed no evidence of intralist proactive interference. The combined results of the seven experiments illustrate some similarities and differences between forgetting in primed fragment completion and in episodic memory tasks such as recall and recognition.

In the word-fragment completion task, subjects are shown words from which certain letters are deleted, for instance _AR_VA___, and they are asked to identify the unique word that fits the fragment, in this case AARDVARK. Fragment completion is a semantic memory task inasmuch as the task could be performed in the absence of, or without necessary reference to, a previous study presentation. However, performance on this task is facilitated for words that subjects have encountered in a previous study episode. Such facilitation will be referred to as direct priming; its effect is measured in terms of the difference between proportions of completion of fragments of studied and nonstudied, or *primed* and *unprimed*, words.

The term *priming* in this context is derived from the pioneering work of Segal and Cofer (1960). They used the term to describe the phenomenon that the frequency of responses on an association test is increased after these responses have been read to subjects. "Direct priming" distinguishes the facilitation resulting from the presentation of the targets themselves from what Cramer (1965) called "indirect priming," the facilitation resulting from the presentation of items that are associatively *related* to targets.

An interesting feature of direct priming effects in word-fragment completion is the relatively slow rate at which retention declines over time. Tulving, Schacter, and Stark (1982) found that a single presentation of a 96-word list produced a sizable priming effect that changed only a little from a test at 1 hr after study to a test given 1 week later. Probabilities of completion of primed words in the two tests were .47 and .46, respectively; corresponding proportions for the unprimed words were .30 and .32. The small reduction in performance in fragment completion over a 1-week retention interval contrasted sharply with forgetting exhibited by the same subjects' yes–no recognition performance over the same interval: The hit rate declined from .78 to .58 and the false-alarm rate increased from .23 to .33.

Little systematic information is available concerning the retention of direct priming effects in word-fragment completion. This article reports seven experiments whose results provide some relevant data. We examined forgetting in primed fragment completion over the first few minutes after study as well as beyond the 1-week interval. Because we found much more forgetting over 1 week than had been observed by Tulving et al. (1982), we made an attempt to identify the source of this discrepancy, unsuccessfully, as it turned out. We also studied retention of primed fragment completion as a function of the nature of interpolated activity and list length.

We begin our investigation with a brief survey of what is known about retention and forgetting in fragment completion and some other tasks in which priming effects have been demonstrated.

Background Information

Extant evidence concerning forgetting in fragment completion is available from five studies in addition to the original Tulving et al. (1982) experiment, as follows: (a) An exact replication of the Tulving et al. experiment was conducted in our laboratory at Toronto in 1982. (b) Light, Singh, and Capps (1986) tested both younger (mean age of 23.4 years) and older (mean age of 69.4 years) subjects immediately

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Steven Sloman is now at Stanford University.

Correspondence concerning this article should be addressed to Endel Tulving, Department of Psychology, University of Toronto, Toronto, Ontario, Canada M5S 1A1.

following study of a 40-word list as well as 1 week later. (c) Komatsu and Ohta (1984) conducted two experiments with Japanese nouns of five or six *Hiragana* letters. In the first, subjects studied a list of 90 words on a single trial, and completed fragments of different subsets of study-list words as well as fragments of unprimed words, after intervals of 8 min, 1 week and 5 weeks. In the second experiment, subjects studied a 75-word list and then took fragment-completion tests for subsets of the studied words after 1 min, 1 hr, and 1 week. (d) Chandler's (1983) subjects studied lists of 12 words and 48 words, and took fragment-completion tests after retention intervals of 1 min and 7 days. (e) Roediger and Blaxton (1987a, Experiment 2) measured primed fragment-completion performance for a 96-word list approximately 15 min and 1 week after study. The results of all these studies are summarized in Table 1.

Komatsu and Ohta (1984), in their first experiment, gave another group of subjects yes–no recognition tests for the same study materials after the same intervals that they used for fragment completion. Recognition performance, expressed in terms of the difference between the hit rate and the false-alarm rate, showed a much larger drop over time than fragment completion, falling from .67 in the first test to .18 in the third.

Although it is difficult to draw clear conclusions from this set of experiments, the following four observations receive some support: (a) primed fragment completion does exhibit forgetting, (b) the amount of forgetting over a given retention interval seems to be quite variable, (c) some of the determinants of the rate of forgetting seem to be list length, age of subjects, and the temporal proximity of the initial retention test to the study episode, and (d) the rate of forgetting in primed fragment completion seems to be slower than in yes–no recognition.

Forgetting in primed performance in other kinds of tasks has shown similar, but as yet little understood, variability. We will briefly mention some relevant evidence, without discussing it in detail. As the relation between direct priming effects in different tasks is only beginning to be studied (Blaxton, 1985; Kirsner, Milech, & Standen, 1983; Squire, Shimamura, & Graf, 1987), the generalizability of findings on priming in one task to those in others is uncertain.

Jacoby (1983a) reported no difference in the performance of the tachistoscopic word identification task when tests were given 24, 48, 72, or 96 hr after study, although priming effects were clearly present at all test intervals. In a similar tachistoscopic identification task, Jacoby and Dallas (1981) observed no statistically reliable reduction in primed word identification performance on an immediate test, one given 15 min after study, and one given 24 hr after study. Warrington and Weiskrantz (1978, Experiment 3), investigated word-stem completion in amnesic and control patients, with stems allowing only a single completion (e.g. JUI-- for JUICE, ANK-- for ANKLE). They found no reliable reduction in the priming effect from a 1-hr test to a 24-hr test in either group. Using mutilated words as recall cues (words whose constituent letters have been partially obliterated), Woods and Piercy (1974) found a priming effect in subjects' ability to name the mutilated words, but no difference in the effect between a test given 1 min after study and one given 7 days after study. All of these experiments have produced data showing little reduction in priming effects over time.

Table 1
Proportions of Primed and Unprimed Fragments Completed at Various Retention Intervals in Experiments That Have Examined the Time Course of Word-Fragment Completion Performance

Condition	Retention interval			Forgetting over 1 week
Tulving, Schacter, and Stark (1982)				
	1 hr	1 week		
Primed	.47	.46		
Unprimed	.30	.32		
Difference	.17	.14		.03
Tulving, Schacter, and Stark (1982) replication				
	1 hr	1 week		
Primed	.49	.46		
Unprimed	.27	.30		
Difference	.22	.16		.06
Light, Singh, and Capps (1986)				
	Immediate	1 week		
Younger subjects				
Primed	.55	.39		
Unprimed	.30	.31		
Difference	.25	.08		.17
Older subjects				
Primed	.46	.41		
Unprimed	.31	.33		
Difference	.15	.08		.07
Komatsu and Ohta (1984)				
	8 min	1 week	5 weeks	
Experiment 1				
Primed	.43	.40	.30	
Unprimed	.16	.22	.21	
Difference	.27	.18	.09	.09
	1 min	1 hr	1 week	
Experiment 2				
Primed	.42	.41	.38	
Unprimed	.22	.23	.21	
Difference	.20	.18	.17	.03
Chandler (1983)				
	1 min	1 week		
List length 12				
Primed	.62	.33		
Unprimed	.18	.19		
Difference	.44	.14		.30
List length 48				
Primed	.42	.34		
Unprimed	.26	.26		
Difference	.16	.08		.08
Roediger and Blaxton (1987a)				
	20 min	1 week		
Primed	.51	.40		
Unprimed	.22	.26		
Difference	.29	.14		.15

Note. Differences between primed and unprimed performance are presented as measures of the priming effect. Forgetting is the difference between priming effects obtained at some initial test and after an interval of a week.

Other experiments, however, have exhibited larger reductions in performance on priming tasks over time. Thus, for instance, Scarborough, Cortese, and Scarborough (1977) reported data showing that primed performance in the lexical decision task is lower after 48 hr than immediately after study. In two separate experiments, Jacoby (1983a) found primed tachistoscopic word-identification to be lower in tests given 24 hr after study than in immediate tests. Relatively rapid forgetting has also been demonstrated in stem-completion tasks in which stems permit multiple alternative responses. Graf and Mandler (1984) measured stem completion immediately after study, 20 min later, and 90 min later. Although the priming effect was still present at 90 min, it was greatly reduced from that observed immediately after study. Graf, Squire, and Mandler (1984), using the same stem completion task, found a smaller priming effect 15 min after study than immediately after study. Two hours after study, no priming at all was observed. Shimamura and Squire (1984) reported similar results with alcoholics and patients with Korsakoff's syndrome, in both stem completion (Experiment 2B)—see also Diamond and Rozin (1984)—and free association (Experiment 4). In an earlier experiment, Martin (1964) found that priming effects in a free-association task were fully dissipated when 20 interpolated words had appeared between study and test of a given word. In the experiment by Woods and Piercy (1974) mentioned earlier, recall cued with two-letter stems of target words was considerably lower after 1 week than it was 1 min after study.

Our survey shows that, like forgetting in primed word-fragment completion, reduction of priming effects over time in other tasks shows a good deal of as yet little understood variability. Further study of the problem is clearly indicated.

The general objective of our experiments was to obtain evidence relevant to the problem of forgetting in primed fragment completion. The results of some of the experiments we reviewed (e.g., Chandler, 1983; Light et al., 1986) suggested that a good deal of forgetting may occur immediately after the study of an item.[1] A pilot experiment and Experiment 1, therefore, examined how rapidly forgetting occurs very shortly after study. Komatsu and Ohta's (1984) second experiment suggested that priming effects were still present 5 weeks after study. In our Experiment 2 we extended the retention interval to 16 months, in an attempt to find out how long direct priming effects do last. Experiments 3 and 4 grew out of Experiment 2, as attempts to solve a problem posed by the results of that experiment. Having found pronounced forgetting in the first five experiments, we next wondered about retroactive and proactive inhibition in the task we had studied. The last two experiments, therefore, were designed to provide evidence concerning the effects of prior and interpolated activity, and list length, not only on primed fragment completion but also, for purposes of comparison, on episodic recognition.

Pilot Study

The purpose of the pilot study was to make a preliminary investigation into the course of forgetting in primed fragment completion over the first hour or so after study. It is reasonable to assume that immediately after the presentation of a single word the probability of successful completion of its fragment is near unity. When study words are presented in a list, however, the retention interval of individual items varies, depending upon the length of the list and the position of a given item in the study and test sequence. In the pilot study, probability of successful fragment completion was measured as a function of the intratrial retention interval (Tulving & Arbuckle, 1963) of individual study items.

Subjects saw a long list of words on a single study trial, and were then given three successive fragment completion tests on these primed words as well as on some new unprimed words. In the first test, which followed the presentation of the study list without any interruption, fragments of primed words were presented in the reverse order of their appearance in the study list. Subjects' ability to complete the fragment of the last studied word was tested immediately, without any break, in the first position of the test series. Other test fragments were presented in an order that created increasingly longer retention intervals between the appearance of an individual study item and the appearance of its fragment in the test sequence. The procedure made it possible to plot the time course of retention of studied items in the first few seconds and minutes after study. The second of the three tests was commenced 10 min after study, and the third one 20 min after study.

Method

Subjects were tested individually. They all went through the same procedure. Each subject saw a sequence of 96 words from the pool used by Tulving et al. (1982). Each of these words permits only a single legitimate completion. The words were presented one at a time by means of a slide projector, at a rate of 5 s/word. Immediately after the presentation of the study list, the first of the three successive tests commenced. A deck of 3 × 5 in. index cards containing printed fragments was given to the subject with the instructions to complete each fragment with a meaningful word. The test deck contained 48 test fragments, 32 corresponding to primed words and 16 to unprimed words, selected from the same pool. The fragments of primed words appeared in a "skipping reverse order." Specifically, the order of appearance of these fragments in the immediate test sequence was 96, 93, 90 . . . 9, 6, and 3, where each number represents the serial position of the word in the study list. Interleaved with the fragments of studied words were those of unprimed words. Specifically, each third test fragment was that of an unprimed word. Subjects were allowed up to 10 s to complete each fragment.

Subjects were informed of the general sequence of events before the presentation of the study list. They were told that although some of the fragments in the test sequence would correspond to study-list words, their task was to try to complete each fragment with an English word regardless of whether they remembered having seen the word in the study list or not.

Ten minutes after study, and a few minutes after completion of the first test, the second test commenced. The test order was again the "skipping reverse order," but this time beginning with the study item from the 95th serial position: 95, 92, 89 . . . 8, 5, and 2. Otherwise the procedure was identical. The third test commenced 20 min after study, a few minutes following the end of the second test, beginning with item 94: 94, 91, 88 . . . 7, 4, and 1. During the short intervals

[1] In a post-hoc analysis of the "immediate" test results of the 48-word lists, Chandler (1983) found primed performance to be higher on the fragments tested, on average, 3 min after study (.51) than on fragments tested 7, 11, or 15 min after study (.40).

between tests the experimenter engaged the subject in light conversation.

Three different study lists were constructed from a pool of 144 words by randomly dividing the pool into three sets—A, B and C—of 48 words and assigning two different sets to a list: A + B, B + C, and A + C. The set that was not part of a given study list provided the fragments of unprimed words for that list. The words in each study list were presented in one randomly determined fixed order. Different subjects started the study of their list at different serial positions. The starting points were spaced evenly among the 96 positions. The very first test fragment for every subject was always that of a studied item. For one half of the subjects, the first new test fragment appeared in the second test position, and for the other half in the third.

A total of 18 volunteer subjects participated in the experiment. They were mainly graduate students in the Department of Psychology at the University of Toronto, although the sample also included a few faculty members and postdoctoral fellows.

Results and Discussion

At each test position, each of 18 subjects was tested with a different fragment. With only 18 observations per position, the levels of performance showed great variability. This variability was somewhat reduced by combining data over successive blocks of four test positions.

A graphic summary of the results thus tabulated is presented in Figure 1. The principal impression gained from the inspection of the results is a great deal of "noise," although the first couple of data points do seem to provide a somewhat more regular picture. The forgetting curve we have fitted by eye to the data in Figure 1 shows a systematic decrease in fragment completion performance over the first three blocks, reaching what we think of as asymptotic performance by the fourth block.

The curve represents our somewhat adventurous guess at the order that may lie hidden behind the apparent lack of order. The proportions of completion of fragments of unprimed words (not shown in Figure 1) were equally irregular across test positions. They averaged .24, .32, and .27 in the three tests, respectively, suggesting a larger priming effect—the difference between primed and unprimed words—in the third test than in the second. As we are not willing to accept the notion of "reminiscence" in primed fragment completion, we regard this difference as part of the noise, together with other fluctuations in the data. In the absence of a more reasonable interpretation, we think of the fluctuation of the data points in the second and third tests as noise.

Considering the initial part of the forgetting curve in greater detail, we found that the first test fragment, corresponding to the very last studied word, was completed by 15 subjects out of 18 for a performance level of .83. The completion rate for the other three fragments in the first block of four were, in order, .83, .67, and .61. The four fragments in the second test block yielded completion rates of .67, .50, .78, and .67, in the order of testing. Thus, the data depicted in Figure 1 suggest that the initial sharp drop in fragment completion performance had run its full course in no more than three test blocks of four items. Because the 12th primed-word test fragment was that of the target word seen in the 63rd serial position in

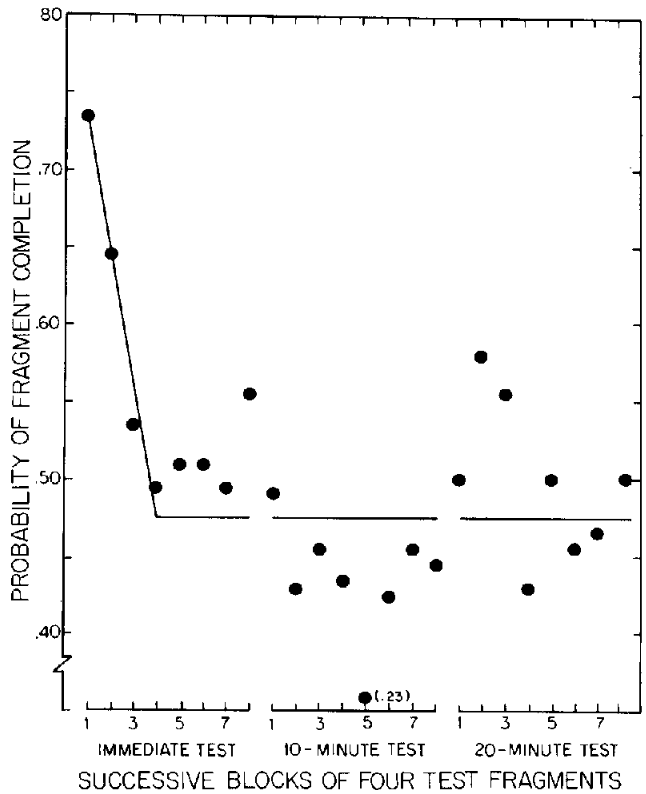

Figure 1. Proportion of word-fragments completed as a function of study–test interval (number of items intervening between study and test of each word) on the 10-min and 20-min delayed tests of the pilot study. (Data have been pooled over groups of four test positions.)

the study list, the maximum length of the intratrial retention interval of that item, on the assumption that the subjects always took their maximum 10 s for responding (no actual times were recorded) was on the order of 5 min. We can conclude, therefore, that in this pilot study the forgetting curve approached an asymptote after approximately 5 min of interpolated activity consisting of study and test of other items.

With the first 12 test items excluded, the mean level of fragment completion performance of the primed words in this experiment was .48. This value is very close to the proportion of primed items completed in the Tulving et al. (1982) experiment after a 1-hour retention interval, namely .47. Because of differences in the experimental procedure and subjects, and because of the small number of observations in the present study, this agreement may be purely coincidental. On the other hand, it is not impossible that much of the reduction in performance from the hypothetical ceiling to that observed an hour after study takes place in the first 5 min or so, if the retention interval is filled with the study and fragment-completion testing of other words.

In summary, the results of the pilot study suggest that intratrial forgetting in fragment completion performance is largely limited to the first 5 min or so after study. Experiment 1, therefore, was undertaken to examine such immediate forgetting in somewhat greater detail.

Experiment 1

The general plan of Experiment 1 was the same as that of the pilot experiment: to test studied words in the reverse order of their presentation. Because forgetting in only the first few minutes was of interest, list length was reduced to 16. Unprimed test items were omitted, thereby making it possible to sample the length of retention intervals more densely. Each subject was tested with eight successive lists, thus eliminating the possibility that whatever forgetting was observed would apply only to experimentally naive subjects. Furthermore, three different replications of the experiment—referred to as Experiments 1A, 1B, and 1C—provided information about the effect of subjects' explicit knowledge of the procedure on their performance.

The three replications of Experiment 1 followed the same general plan, with small differences in procedure, as indicated.

Method

Subjects in Experiment 1A were tested individually. After seeing 16 words in a study list, presented one at a time, at the rate of 3 s/word, they were shown the fragments of the study-list words in the reverse order: The first test fragment was that of the last word presented and the last test fragment that of the first word presented.

A pool of 128 words, and corresponding fragments with single completions, was selected from the larger pool used in the pilot experiment by eliminating all words whose fragments had had very low or very high completion scores in that experiment. Each word was randomly assigned to one of the eight lists. Each list served an equal number of times in each of the eight list-presentation positions, and each word served once at each of the 16 serial positions within a list.

At the beginning of each session, subjects had the fragment completion task explained to them and were shown a fragment as an example. Words were presented by a slide projector. At test, subjects turned over cards in a deck. Each card displayed a test fragment. The first card was blank; it was removed by the subject immediately after presentation of the study list. Subjects had been instructed to proceed to the next test fragment immediately after calling out the correct completion. Otherwise the experimenter allowed the subject 6 s for each fragment before giving a signal to move to the next one. This short interval was used to encourage subjects to respond with the first word that came to mind by forcing them to respond quickly. Instructions were different from those used in the pilot experiment. Subjects were explicitly told at the beginning of the session that all test fragments would belong to the words in the study list seen immediately before each test trial, and that they were to try to complete each fragment with a study-list word.

A total of 16 volunteer graduate students and research assistants in the Department of Psychology participated in Experiment 1A.

The method of Experiment 1B was identical with that of 1A, except as follows. The 16 subjects were introductory psychology undergraduate students, who participated in return for course credit. Their general level of sophistication with respect to fragment-completion task was presumably lower than that of the graduate students and research assistants who participated in Experiment 1A. Study-list words and test fragments were presented on an Apple IIe computer. The first test fragment appeared on the screen immediately after the presentation of the last study item. The word pool used to construct the lists was selected from the words used by Tulving et al. (1982) by excluding the most frequently and least frequently completed fragments. Although there was a large overlap between this pool and the words used in the previous experiments, it was selected independently.

The method of Experiment 1C was identical with that of 1B, except as follows. The 16 subjects were undergraduate students who were paid for their services. They were given explicit instructions as to the nature of the relation between the presentation and the test order. These instructions emphasized the reverse order of testing, and the fact that the last item studied would be the first one tested. To enhance the subjects' understanding of the design of the experiment, a practice list was used to acquaint them with the procedure and to demonstrate the relation between study and test items.

Results and Discussion

In none of the three replications of Experiment 1 was there any evidence for systematic changes in performance across the eight successive lists. With the data pooled over the three subexperiments, the proportions of fragments completed in Lists 1 through 8 were .81, .78, .78, .79, .80, .81, .80, and .78. All the findings reported, therefore, are based on the data pooled over all lists.

The upper curve in Figure 2 presents a graphic summary of the main results of Experiment 1. The proportion of fragments completed is shown as a function of *study-test interval*, expressed in terms of the number of other study and test items interpolated between the presentation and fragment-completion test of a given study-list word. Because performance at all intervals, with the exception of interval 0, was similar across all three versions of the experiment, the data for intervals other than 0 have been collapsed over the three versions. Hence, each of the data points in Figure 2 for Experiment 1, save that of interval 0, is based on 384 observations (16 subjects × 8 lists × 3 replications). Each of the three data points at interval 0 is based on 128 observations.

The main finding of Experiment 1 was intratrial forgetting (cf. Tulving & Arbuckle, 1963) in primed fragment completion. The following five qualifications and elaborations of this finding are suggested by further analyses of the data.

First, the mean probability of fragment completion was very similar in the three replications: .79 (*SD* = .07), .80 (*SD* = .08), and .79 (*SD* = .06) in Experiments 1A, 1B and 1C, respectively. Thus, it seems that the general level of sophistication of subjects, and the specificity of instructions with respect to the relation between serial positions of the items at study and at test, are not important determinants of overall primed fragment-completion performance.

Second, subjects' knowledge of the exact testing sequence, however, seems to have affected the level of performance of the fragment tested at interval 0: the probability of completion of the first test item was higher in Experiment 1A (.95, *SD* = .08), where relatively sophisticated subjects were used, and in Experiment 1C (.97, *SD* = .10), where highly explicit instructions were used, than it was in Experiment 1B (.84, *SD* = .22). Statistical analysis showed the differences between Experiments 1A and 1B, $t(30) = 1.76$, and Experiments 1C and 1B, $t(30) = 2.11$, to be reliable at the .05 level. It may also be worth noting that in the pilot experiment, whose procedure

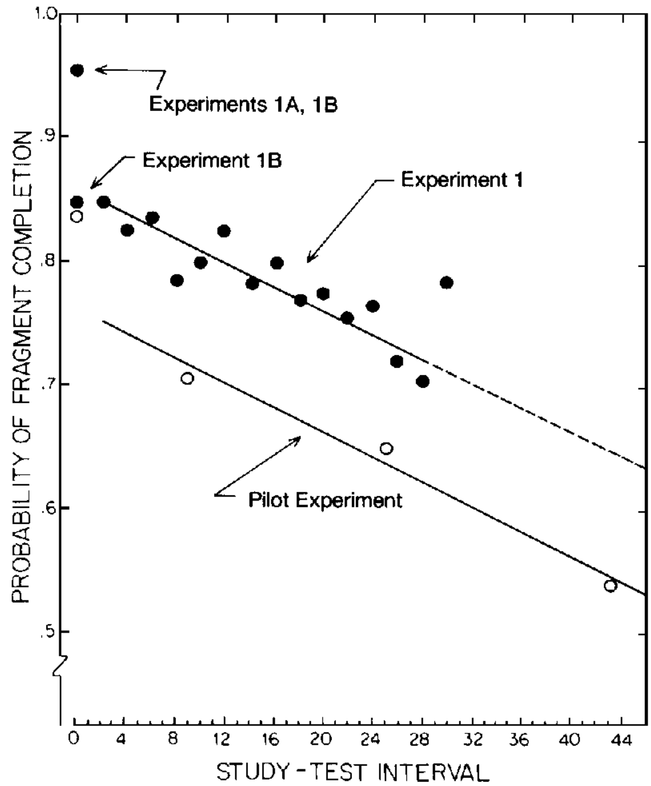

Figure 2. Upper curve: proportion of word-fragments completed as a function of number of intervening study and test items in Experiment 1. (Data points at all intervals except interval 0 are collapsed over Experiments 1A, 1B, and 1C. The least-squares regression line was calculated using intervals 2 through 28.); lower curve: proportion of word-fragments completed as a function of the number of intervening study and test items for the first 12 test positions of the pilot experiment. (The first point represents performance at interval 0; the second point, Test Positions 2 through 4; the third, Test Positions 5 through 8; the fourth point represents performance at Test Positions 9 through 12. The linear fit was calculated on the basis of mean proportions of fragments completed at individual Test Positions from 2 to 12.)

precluded subjects' awareness of the relation between study and test position on the very first fragment, mean performance at interval 0 (.83) was indistinguishable from that in Experiment 1B (.84).

Third, there was evidence of a single-item "primacy effect": The probability of fragment completion of the first word studied, that is, the item tested at interval 30, was higher (.78, SD = .17; .77, SD = .20; and .80, SD = .17, in the three replications of the experiment) than that of other words studied in early serial positions.

Fourth, the forgetting curve described by study-list words from Serial Positions 2 through 15—disregarding the one-word "primacy effect" and the variable performance on the words tested at interval 0—is a linear function of interval with a slope of −.005. That is, the probability of fragment completion is reduced by .01 as the study–test interval increases by 2. These estimates of the slope, based on the method of least squares, were roughly comparable in the three subexperiments: −.0067, −.0029 and −.0041 in 1A, 1B and 1C, respectively. A two-factor split-plot analysis of variance (ANOVA), with the three groups and 14 test positions as variables, confirmed that only the effect of test position was significant, $F(13, 585) = 3.01$, $MS_e = 1.81$, $p < .001$. Neither the group factor nor the interaction between group and test position came close to significance (both Fs < 1).

Fifth, the slope of this linear forgetting curve was similar to the slope of the initial part of the forgetting curve found in the pilot experiment, the part comprising items in Test Positions 2 through 12 (that is, disregarding the test item at interval 0), which was −.0050. This latter curve is also shown in Figure 2, schematically represented by three data points, the first corresponding to Test Items 2 through 4, the second to Test Items 5 through 8, and the third to Test Items 9 through 12. (The data from individual test positions were highly variable.) The difference in the intercepts of the two curves presumably reflects the difference in list lengths.

The shapes of the forgetting functions depicted in Figure 2 are quite different from comparable forgetting functions obtained in situations where retention is tested by free or cued recall. The typical serial position curves in free recall include a primacy effect extending over the first two or three items of the list, pronounced recency effects involving the last six items or so, and a flat middle portion of the curve between them (e.g., Murdock, 1962). Similar bow-shaped serial position curves are sometimes yielded by paired-associate cued recall (e.g., Tulving & Arbuckle, 1963).

On the other hand, the intratrial forgetting function observed in Experiment 1 with fragment completion tests is more similar to forgetting functions obtained with yes–no recognition. For example, Murdock and Anderson (1975, Figure 4) have described data showing that recognition hit rates decline nearly linearly with study–test interval when a small primacy effect is excluded. The slope of Murdock's and Anderson's curve is quite similar to the slopes of forgetting curves for fragment completion in our pilot experiment and Experiment 1 (approximately −.005). Shulman (1970) used lists of 10 words and found a near-linear forgetting curve when recognition hit rates were plotted against study–test interval. In his experiment, however, the slope of the function was steeper, dropping from .97 at interval 0 to .72 at interval 9. The small primacy effect that appeared in Murdock's and Anderson's experiments was also found by Shulman. We should also note that Hockley (1982), using a continuous recognition task, found the hit rate declining bilinearly as a function of study–test interval.

When making these kinds of comparisons it is important to keep in mind the possibility that the forgetting curves may vary with variables held constant in individual experiments but varying between experiments, for instance, the rate of presentation of items at study and the length of time allowed for responses at retrieval.

In conclusion, Experiment 1 confirmed the results of the pilot experiment in providing evidence for approximately linear forgetting in primed fragment completion in the first few minutes after study, at the rate of approximately .005 per interpolated event (study or test of a single word). With respect to the linearity of this forgetting function, it seems that fragment completion is more similar to recognition than it is to free or cued recall.

Experiment 2

The pilot study and Experiment 1 examined the course of primed fragment completion performance in the short term, during the first seconds and minutes after study. Experiment 2 was designed to look at the course of retention in the longer term, over weeks and months. Primed fragment completion performance was tested after six different intervals, from 18 min through 16 months.

Method

Retention interval and priming were the only independent variables in this experiment. A total of 109 subjects were tested after each of five intervals (18 min, 1 week, 5 weeks, 12 weeks, and 23 weeks). Of these subjects, 26 were also tested after a 71-week interval. In the first session, during an introductory cognitive psychology class, subjects were shown a list of 102 words. This study list was composed of 96 words from the Tulving et al. (1982) pool and six pronounceable pseudowords.

The words were printed on two sheets of paper and subjects were instructed (a) to copy each word onto a blank line beside the word, (b) to make a judgment about the familiarity of each word, on a four-point scale, from 0 (*unfamiliar*) to 3 (*highly familiar*), and (c) to prepare themselves for an unspecified "memory test" on the words. They were allowed 12 min for the completion of the study task. The purpose of the subjects' copying of study-list words was to "prime" the correct spelling of the words. The pseudowords were included as "lie-detection" items. The intention was to disregard the data of subjects who claimed familiarity with any of them. As it turned out, no subject had to be rejected for this reason.

Each subject was tested after each retention interval with a different set of 16 primed words, together with an equal number of unprimed fragments. The 32 test fragments were printed on a single sheet of paper. At the top of this sheet were the instructions for fragment completion. Subjects were to go through the fragments twice, spending 5–6 s on each fragment, attempting to complete each with any English word that fit the fragment without using plurals or proper nouns. Subjects were informed that some of the fragments belonged to words that had appeared in the original study list and some others did not. Subjects were allowed 6 min to complete each test.

Because in this experiment students were tested in a class, it was not possible to maintain complete control over the presence or absence of subjects. Therefore, despite the perfectly balanced initial design, the 192 words from the total pool turned out to be only roughly counterbalanced across priming conditions (primed or unprimed) and retention intervals. The first five retention tests were conducted in class. For these tests data were available from 109 subjects who were present on all five occasions. For these 109 subjects, each word from the pool appeared at least 6 times and no more than 13 times in each primed or unprimed test set at any of the five retention intervals. Sixteen months after the original study episode, 26 of these subjects, when contacted by telephone, agreed to participate in the sixth retention test for monetary compensation. These 26 subjects were tested individually over the course of a 2-week period. Before beginning the fragment completion test, they were asked to imagine themselves back in the classroom, sitting in the seat in which they normally sat, surrounded by the people who normally surrounded them. This was done in an attempt to reinstate the contextual conditions of testing that had been present at the other five retention intervals (cf., Smith, 1979). For these 26 subjects, each word from the total pool appeared in each of the 12 conditions at least 1 time and no more than 3 times.

Table 2
Mean Proportions of Fragments of Studied and Unstudied Words Completed by 109 Subjects at the First Five Retention Intervals: Experiment 2

	Retention interval				
Word type	18 min	1 week	5 weeks	12 weeks	23 weeks
Studied					
M	.47	.35	.31	.29	.26
SD	.16	.15	.15	.14	.15
Unstudied					
M	.20	.21	.20	.22	.22
SD	.13	.13	.14	.14	.14
Difference	.27	.14	.11	.07	.04

Results and Discussion

Table 2 presents the proportions of primed and unprimed word fragments completed by all 109 subjects at the first five retention intervals, along with standard deviations. As a means of indicating the magnitude of the direct priming effect, the differences between primed and unprimed performance on each test are also shown. Table 2 shows that there was a relatively systematic decrease in primed fragment completion performance across the first 23 weeks. There was no systematic change in unprimed performance. These conclusions are supported by an ANOVA that yielded significant effects of priming, $F(1, 108) = 400.48$, $p < .001$, retention interval, $F(4, 432) = 19.12$, $p < .001$, and interaction between priming and retention interval, $F(4, 432) = 45.48$, $p < .001$. A one-way ANOVA on new item performance showed no effect of retention interval, $F(4, 432) < 1$. The least significant difference for comparing primed and unprimed performance at the .01 level of significance is .03. By this standard, priming effects were highly significant at all retention intervals.

Table 3 presents data from all six retention intervals collected from the subset of 26 subjects who were tested after 16 months. The pattern of both primed and unprimed performance is quite similar to that of Table 2 for the first five retention intervals. Also, a sizable priming effect was observed

Table 3
Mean Proportions of Fragments of Studied and Unstudied Words Completed by 26 Subjects at Six Retention Intervals: Experiment 2

	Retention interval					
Word type	18 min	1 week	5 weeks	12 weeks	23 weeks	71 weeks
Old						
M	.47	.36	.32	.30	.28	.31
SD	.20	.16	.16	.16	.15	.16
New						
M	.23	.22	.19	.27	.25	.22
SD	.16	.14	.14	.15	.12	.18
Difference	.24	.14	.13	.03	.03	.09

after 16 months. The drop in the priming effect over time is much less systematic than that observed for the full complement of 109 subjects. This is largely attributable to the high variability in new item performance by the smaller group of subjects. An ANOVA yielded significant effects of priming, $F(1, 25) = 83.90$, $p < .001$, retention interval, $F(5, 125) = 2.55$, $p < .05$, and interaction between retention interval and priming, $F(5, 125) = 8.04$, $p < .001$. A one-way ANOVA on new performance over time was nonsignificant, $F(5, 125) = 1.37$. The least significant difference at the .01 level of significance between primed and unprimed performance is .07. The priming effect on the 16-month retention test, as well as at the first three retention intervals, surpasses this critical difference.

The priming effects at the fourth and fifth retention intervals do not achieve the least significant difference of .07. The absence of significant differences on these tests may be attributable to the relatively small sample of subjects, incomplete counterbalancing of words, or both.

We also examined primed fragment-completion performance conditionalized on subjects' familiarity ratings of study-list words. There was no pronounced systematic relation between familiarity ratings and the rate of forgetting. A similar amount of forgetting obtained regardless of the prior familiarity rating.

The major findings to emerge from Experiment 2 are that (a) extended forgetting does occur in primed fragment completion over days, weeks, and months, and (b) the rate of this forgetting is sufficiently slow to make the consequences of a single study episode clearly detectable more than a year later. These findings are reminiscent of Kolers's (1976) observation that subjects' reading speed was greater for typographically inverted text that they had read once 13 to 15 months previously than for the text not read.

Although the group-testing procedure precludes exact statements about the time subjects spent on individual words at the time of study, the maximum estimate of the *average* study time is approximately 7 s per word. The actual figure is probably considerably smaller as many students finished the study task in much less than 12 min. The fact that such a brief encounter with a word, in a long list of other words, can facilitate its fragment completion more than a year later attests to the remarkable powers of whatever memory systems are responsible for direct priming effects in word-fragment completion (cf., Tulving, 1985).

The monotonic decrease in performance on primed words from test to test in Experiment 2 is discrepant with the finding of an asymptote after a few minutes in the pilot experiment, as well as the slow rate of forgetting from 1 hr to 1 week observed by Tulving et al. (1982) and by Komatsu and Ohta (1984, Experiment 2). The first discrepancy does not seem to be particularly troublesome as it may reflect nothing more than the insensitivity of the pilot experiment to detect the relatively slow forgetting after the first few minutes. The second discrepancy is more puzzling as the insensitivity hypothesis does not apply to the Tulving et al. (1982) or the Komatsu and Ohta (1984, Experiment 2) design. It is more likely that certain differences in experimental design and procedure were responsible for this discrepancy. Experiments 3 and 4 were undertaken in an attempt to identify these differences.

Experiment 3

In Tulving et al.'s (1982) experiment, the fragment-completion test for words seen previously only at study, at both 1-hr and 1-week intervals, always followed a *recognition* test of a *different* set of words from the same study list. In Experiment 2, no recognition test at all was used. It is possible, therefore, that in Tulving et al.'s experiment, primed fragment completion performance after a week was enhanced by the immediately preceding recognition test of another part of the study list, perhaps through reinstatement or reactivation of the study list context. Such reactivation might occur because (a) during the recognition test subjects would have to "think back" to the previous study episode, and (b) the physical presence of some study list words on the recognition test may serve as reminders of the previous list-learning episode.

Experiment 3 was designed to test this "list reactivation" hypothesis through a systematic manipulation of activities that preceded the test of fragment completion.

Method

There were three independent variables: presence or absence of priming, retention interval, and reactivation task. The first two were manipulated within subjects, the last one between subjects. A sample of 142 students, members of an introductory cognitive psychology class, participated as subjects. They were tested as a group in two successive weekly sessions. They studied the same list of 96 words, by the same procedure that had been used in Experiment 2, and were then tested for fragment completion of a subset of 16 study-list words and 16 unprimed words on the first test, given an hour after study, and on a subset of 32 study-list words and 32 unprimed words on the second test, given 1 week later. A "reactivation task" preceded the fragment completion test in the second session.

Two kinds of study booklets were prepared to balance the primed and unprimed test words between the 1-hr and 1-week tests. Half of the subjects studied one list and half studied the other. Subjects were allowed up to 12 min to copy the 96 words and to rate them for familiarity, as in Experiment 2. An hour later, each subject attempted the same fragment-completion test, composed of 16 words seen in the study list and 16 lures. Subjects were allowed 6 min to complete the test. One week later, each subject was assigned to one of the five reactivation conditions through a haphazard distribution of test booklets in the class, except that they were required to take a test booklet that corresponded to the study group they had been in the previous week. Each test booklet consisted of two parts. The first provided the "reactivation" or control treatment. For three of the five groups it consisted of 48 recognition-test items: Either all 48 test items were from the study list, 24 were from the study list and 24 were new, or, for the third group, all 48 test items were new. For the fourth group the first part of the booklet consisted of 50 geography questions. For the fifth group the first part of the booklet consisted of a fragment-completion test composed of 32 words seen in the study list and 32 new words. All subjects were allowed 6 min to complete the test. The second part of the test booklet was identical for all subjects. It consisted of the same word fragments and lures presented to the fifth group while the other four groups were in the reactivation phase. Subjects were allowed 12 min to complete the fragments. The fifth group were told not to look back at their previous attempt on the test.

Results and Discussion

The mean proportions of primed and unprimed fragments completed by each of the five groups at both retention intervals are shown in Table 4. Table 4 also shows the amount of forgetting in each group, defined as the difference between priming effects observed after an hour and after a week. (Priming effect refers to the difference between performance on primed and unprimed items.)

The data in Table 4 show that an appreciable amount of forgetting occurred over the course of 1 week (mean of .15), and that the nature of the immediately preceding task had no systematic effect on primed fragment completion performance. The smallest amount of forgetting occurred in the group without any "reactivation" treatment, completely contrary to the hypothesis, but the effect of treatment group was not statistically significant. A three-way ANOVA, with type of words (primed vs. unprimed), treatment groups, and retention interval as independent variables yielded significant effects for priming, $F(1, 137) = 596.56$, $MS_e = 1.88$, $p < .001$, and retention interval, $F(1, 137) = 295.41$, $MS_e = 1.83$, $p < .001$, and the interaction between retention interval and priming, $F(1, 137) = 99.19$, $MS_e = 2.11$, $p < .001$, but not for treatment groups or for the interaction between treatment groups and retention interval, $Fs < 1.0$. The data thus fail to replicate Tulving et al.'s (1982) finding of negligible forgetting over 1 week, and provide no support for the reactivation hypothesis as an explanation of the discrepancy between the outcomes of different experiments. It seems that a recognition test on one set of study-list items does not reduce the rate of forgetting observed in fragment completion of another set.

Table 4
Mean Proportion of Primed and Unprimed Fragments Completed as a Function of Reactivation Task and Retention Interval: Experiment 3

Reactivation task	Retention interval: 1 hr Primed	1 hr Unprimed	1 week Primed	1 week Unprimed	Forgetting
Recognition tests					
All old	.46	.18	.23	.14	.19
Half old, Half new	.42	.18	.24	.14	.14
All new	.46	.17	.27	.15	.17
Geography test	.40	.17	.20	.12	.15
No task	.44	.20	.24	.12	.12
M	.44	.18	.24	.13	.15

Note. Forgetting refers to the difference between primed and unprimed fragments at the 1-hr test minus the difference between them after 1 week.

Experiment 4

Because our initially promising reactivation explanation of slow forgetting in primed fragment completion in Tulving et al. (1982) foundered on the findings of Experiment 3, we next concentrated on another potentially important variable, namely, the format of presentation of study list words. In our Experiments 2 and 3, as well as in Roediger and Blaxton's (1987a) Experiment 2, in which sizable amounts of forgetting in primed fragment completion over 1 week had been observed, study list words were either typed or handprinted on sheets of paper. Tulving et al. (1982), on the other hand, presented stimuli on slides, projected onto a screen at the front of a room, giving the words an unusually large appearance. If physical attributes of study-list words play an important role in priming, as has been suggested (e.g., Flexser, 1986; Jacoby, 1983b; Roediger & Blaxton, 1987b), physical attributes of words may also be important determinants of whatever proactive and retroactive interference effects may be reflected in forgetting of primed fragment completion. Thus, it is conceivable that study-list words read from sheets of paper may be susceptible to interference from small typed and printed words encountered in the reading material in everyday life whereas large white words projected on a deep blue background are not. Experiment 4 was conducted to test the hypothesis that more forgetting in primed fragment completion obtains when study stimuli are printed on paper than when the study list is shown on slides.

The intention of testing this hypothesis was to resolve the discrepancy in findings of forgetting between Tulving et al. (1982) and Experiments 2 and 3. Therefore, two study tasks were administered to two different groups which conformed closely to, in the case of the slide group, the study task used by Tulving et al. and, in the printed-sheet case, the study task used by Experiments 2 and 3. This led to a confounding between presentation format and encoding activity in that the printed-sheet group copied the words and made familiarity judgments about them whereas the slide group simply watched the presented words.

Method

There were three independent variables in this experiment, all crossed factorially: format of study-list presentation, retention interval, and priming. The presentation format was manipulated between subjects, the other two variables within subjects. Retention interval was either 30 min or 1 week.

The experiment was conducted in the same introductory cognitive psychology class that had participated, 4 months earlier, in Experiment 3. A total of 99 students were present for testing at both retention intervals. The study-list words were selected from a pool constructed by Chandler (1983), containing words different from those used by Tulving et al. (1982) but still permitting only unique completions. One group of 53 students, whose names began with letters in the first half of the alphabet, saw a list of 96 words projected on a large screen at the front of the classroom at a rate of 5 s per word. Their instructions were to study each word carefully in preparation for the fragment completion test. This was the procedure used by Tulving et al. The other group of 46 subjects saw the same words, in the same order, printed on two sheets of paper, each sheet listing three columns of 16 words each. Subjects were instructed to copy each word and rate it for familiarity, as in Experiments 2 and 3, proceeding through the list in keeping with an auditory signal heard at five second intervals. They were also told that they would be tested for fragment completion of these words.

For fragment completion testing, all subjects were given a single sheet of paper with 48 fragments (24 primed, 24 unprimed) in a random order. Four different test forms were used, to sample all primed words and to counterbalance test fragments between the two tests. In the slide group, the four test forms were used 11 to 15 times each; in the printed-sheet group, different test forms appeared anywhere from 10 to 13 times. Because the slide group was shown the study list all at the same time, and the printed-sheet group was given exactly the same list, it was impossible to counterbalance primed and unprimed items. Therefore, two different sets of 24 words were selected to act as a source of unprimed fragments. Each set of new items appeared on the test forms of either 49 or 50 subjects in both the early and delayed tests.

Results and Discussion

Table 5 shows means and standard deviations of proportions of both primed and unprimed old fragments completed at each of the two retention intervals for the two groups. As can be seen, the priming effect was considerably smaller after 1 week than after 30 min, and about equally so for both groups.

An ANOVA showed that there was, as usual, a highly significant effect of priming, $F(1, 97) = 272.22$, $p < .001$, as well as retention interval, $F(1, 97) = 6.78$, $p < .05$. The mean performance dropped from .45 after 30 min to .35 after 1 week. The interaction between retention interval and priming was also highly significant, $F(1, 97) = 43.83$, $p < .001$. But F ratios were smaller than unity for presentation format and for the interaction between presentation format and retention interval. Thus, it seems that differences in the format of presentation of the kind examined in this experiment have no effect either on priming in fragment completion or on the rate at which such priming effects decline over time. Encoding activity was confounded with presentation format, and this allows the possibility that the encoding activities counteracted any effects on the presentation format as such. But in our opinion the possibility is farfetched.

The results of the experiment offer no support for the hypothesis that differences in study task (presentation format and encoding activity) are responsible for discrepancies in forgetting rates over a week's interval. The results of Tulving et al. (1982) once more failed to be replicated. The discrepancy between them and those from other experiments remain unexplained. If it were not for Komatsu and Ohta's (1984) Experiment 2, we would have to conclude that in the light of the results of our Experiments 2, 3, and 4, as well as those of Roediger and Blaxton (1987a, Experiment 2), the finding of little forgetting over a week of Tulving et al. may have represented a relatively unique occurrence. Now a more reasonable conclusion seems to be that the rate of forgetting in primed fragment completion seems to be sensitive to an as yet unidentified variable or variables.

Experiment 5

Given that we had observed forgetting in all five experiments described so far, and given the long tradition of attempts to understand forgetting in terms of interference effects (e.g., Crowder, 1976; Postman, 1971), it seemed reasonable to wonder about the extent to which forgetting in primed fragment completion might be attributable to interference from other material encountered by subjects before and subsequent to the study of target items. Experiments 5 and 6 were undertaken to examine the roles of retroactive and proactive interference in fragment completion.

Although so far there have been no published reports concerning interference effects in primed fragment completion, Jacoby (1983a) has examined interference effects using another task in which priming occurs. He presented to subjects a different word list on each of five successive days and subsequently tested these words for tachistoscopic word identification. The presentation produced sizable priming effects, but no evidence was obtained for either proactive or retroactive interference.

Experiment 5 was designed to investigate the effects of retroactive interference in fragment completion. Before describing it, we briefly mention the results of a related pilot study. In the pilot study, 48 subjects were shown a list of 36 words on a single study trial, and were then tested for fragment completion at three intervals: immediately, 10 min after study, and 20 min after study. At each test, fragments of one third of the study list words plus an equal number of unprimed fragments were used. Interpolated between the first and the second test were four different kinds of activity, each engaged in by 12 subjects: (a) study and free recall of five lists of words semantically similar to words in the study list, (b) study and free recall of words orthographically similar to study-list

Table 5
Mean Proportions of Primed and Unprimed Fragments Completed and Differences Between Primed and Unprimed Performance as a Function of Study Task and Retention Interval: Experiment 4

Study group	30-min test Primed	30-min test Unprimed	30-min test Difference	1-week test Primed	1-week test Unprimed	1-week test Difference
Slides						
M	.45	.22	.23	.35	.26	.09
SD	.17	.15		.17	.14	
Booklet						
M	.45	.23	.22	.35	.24	.11
SD	.16	.10		.16	.10	

words, (c) study and free recall of lists of words neither semantically nor orthographically similar to study-list words, and (d) a nonverbal activity, namely, a video game. The results of the pilot study showed that fragment completion performance was higher in the immediate test (.75) than in either the second or third tests (both .57), and that the nature of interpolated activity had no effect on performance at the two retention intervals examined.

Experiment 5 was a simplified but extended version of this pilot study. Subjects studied a list, spent the next 20 min in either a verbal or nonverbal interpolated activity, and were then tested for fragment completion. Because we had discovered in Experiment 1 that the retention function of primed fragment completion was similar to that of episodic recognition, the design of Experiment 5 also included a further two groups of subjects who, after engaging in the same interpolated activities, were tested for the study material by a two-alternative forced-choice recognition test.

Method

Two independent variables were manipulated between subjects: type of interpolated activity (verbal or nonverbal) and type of test (fragment completion or recognition). Priming was varied within subjects. A total of 64 subjects, undergraduate students at the University of Toronto who were paid for their participation, were randomly assigned to each of the four groups, with 16 subjects per group. In the verbal interpolation condition, the interpolated activity consisted of the successive study and recall of 12 lists, each comprised of 12 unrelated words. In the nonverbal interpolation condition, subjects spent an equivalent amount of time playing a video game.

Four different critical study lists of 25 words were selected from Tulving et al.'s (1982) pool, avoiding the use of words whose completion probability had been very low or very high in previous studies. Four subjects in each group studied one of the four lists. For each of these subgroups, a different list served as unprimed items on the fragment completion test or as distractor items on the recognition test. The 144 seven-letter words that made up the 12 interpolated lists in the verbal interpolation conditions were selected from a puzzle solver's handbook. An attempt was made to avoid words similar in meaning or in orthography to the words in the critical study lists.

Subjects were tested individually. At the beginning of the session, the subjects in the fragment completion groups received instructions regarding the fragment completion task; they were also given five practice fragments to acquaint them with the task. The subjects in the nonverbal interpolation groups were told that they would be playing a video game later on in the experiment. The game was described and the subjects were given 30 s of practice on it. The object of the game was to destroy as many "meteorites" as possible with a "spaceship" while avoiding having the spaceship struck by the meteorites. The game got progressively more difficult as more meteorites were destroyed. Following these preliminaries subjects were told that they would be shown a list of words. They were instructed to pay close attention to the words, since their memory for them was going to be tested. The form of the test was not specified.

The study-list words were presented on the screen of an Apple IIe computer, at the rate of 1 s/word. Four primacy buffer words preceded, and three recency buffer words followed, the presentation of the 25 critical words. Following the presentation, subjects were advised that they would next engage in a somewhat different activity. In the verbal interpolation conditions, subjects were told that they would be shown lists of words, and that they would have to recall these words immediately after their presentation. The 12 lists were then shown, with the words presented at a rate of 3 s/word, and subjects were allowed 1 min for written free recall after each list. Subjects in the nonverbal interpolation groups played the video game for a comparable length of time (20 min).

Following the interpolated activity, the subjects taking the fragment completion test were shown the fragments of 50 words, 25 primed and 25 unprimed, in random order on the computer screen, and were allowed up to 10 s to say out loud the completion for each fragment. Instructions were to complete each fragment with an English word, avoiding proper nouns and plurals, if possible; no mention was made of primed words in the test. The subjects taking the two-alternative forced-choice recognition test were shown 25 pairs of test words, again in random sequence. The words, one from the study list and one not previously seen in the experiment, were placed side by side, the side being determined randomly, on the video screen. Instructions were to indicate by a key press, guessing if necessary, the word that they had seen in the study list. The recognition test was unpaced.

Results and Discussion

The results of Experiment 5 are summarized in Table 6. It shows mean proportions of correct responses to the fragments of primed and unprimed words, differences between primed and unprimed performance, as well as mean proportions of correct choices in the two-alternative forced choice recognition test, following the two kinds of interpolated activity, together with standard deviations of these means.

Statistical tests confirmed the impression that fragment completion was not affected by the type of interpolated activity. An ANOVA showed that fragment completion performance varied only for the primed versus unprimed words, $F(1, 30) = 54.11$, $p < .001$. Neither the nature of interpolated activity nor the interaction between interpolated activity and the priming status approached significance ($Fs < 1$). The conclusion follows, therefore, that retention measured by fragment completion is not affected by large amounts of interpolated verbal activities of the kind used in the experiment: the study and recall of 12 lists of 12 words each. These results are similar to those found by Jacoby (1983a) in primed tachistoscopic word identification.

The nature of interpolated activity had no reliable effect on performance in the two-alternative forced-choice recognition, either. Although the proportion of correctly chosen items was somewhat higher following nonverbal interpolation (.89) than following verbal interpolation (.84), the difference was not

Table 6
Mean Proportions Correct for Primed and Unprimed Fragment Completion, Differences Between Primed and Unprimed Performance, and Two-Alternative Forced-Choice Recognition as a Function of Interpolated Activity: Experiment 5

Interpolation	Primed	Unprimed	Difference	Recognition
Verbal				
M	.57	.36	.21	.84
SD	.14	.14		.10
Nonverbal				
M	.59	.34	.25	.89
SD	.13	.13		.11

statistically significant, $t(30) = 1.23$, $p > .10$. This finding of small but unreliable retroactive inhibition seems typical of the results obtained by other investigators in experiments on recognition. Thus, Gibson (1934), McKinney (1935), and Postman (1952) all reported slightly lower recognition scores after interpolation of material to the critical material, compared with appropriate control tasks. Zangwill (1938) found that geometric figures embedded in larger patterns could be identified more readily by subjects who heard a lecture interpolated between study and test than by subjects who studied and recalled two different sets of complicated figures between study and test. Deffenbacher, Carr, and Leu (1981) found both statistically significant and nonsignificant retroactive interference effects, using faces, landscapes, line drawings, and concrete nouns as to-be-remembered items in recognition. The test they used was different from the one used in our experiment in that theirs included a discrimination judgment between the critical and interpolated list. All these findings do suggest that retroactive interference does occur in recognition of single items although the effects may be unreliable when small samples of subjects are used.

On the basis of the results of Experiment 5, we conclude that fragment completion performance is somewhat less susceptible to retroactive influences than is recognition memory; however, these tasks are similar in that massive interpolation of verbal materials has much less of a debilitating effect on performance in them than in free recall (Tulving & Thornton, 1959), in serial learning (Melton & Irwin, 1940), and with paried associates (Barnes & Underwood, 1959). It is possible to argue, of course, that the interpolation of verbal materials might have been more effective in producing retroactive inhibition in primed fragment completion if the interpolated materials had been more similar to the critical materials. But we tend to be skeptical about such a hypothesis, because of the results of the pilot study briefly described earlier. In that study, there was not even a hint of any effects of either semantic or graphemic similarity.

Our findings do not mean, of course, that retroactive inhibition could not be found in primed fragment completion performance under different conditions. For instance, it is conceivable that such effects might be observed when testing takes place relatively soon after study, in the first minute or so. It is also conceivable that engaging subjects in interpolated activities that are *procedurally* more similar to the fragment completion task might show effects where study and free recall of the words did not. But these and other possible further tests must await future research. For the time being our tentative conclusion holds: Studying and recalling verbal materials does not produce retroactive inhibition in primed fragment completion.

Experiment 6

Experiment 6 was designed to answer two outstanding questions. First, the finding of no effect of retroactive interference on fragment completion in Experiment 5 leads us to wonder if we could find any proactive effects. Second, despite the many methodological differences between the initial pilot experiment and Experiment 1, the linear portions of the two forgetting curves yielded very similar slopes. Because the most striking difference between the experiments was the length of the study lists (96 items in the pilot experiment and 16 items in Experiment 1), we thought it advisable to confront the issue of the effect of list length on primed fragment completion performance directly. Hence, in Experiment 6, we compared fragment completion performance for 10-word and 100-word lists following a 10-min retention interval filled with identical activity in both conditions. By comparing the retention performance on the short list with that of the comparable set of items studied at the end of the long list we could measure intralist proactive effects. By comparing the overall performance on the two lists, we could measure the effects of list length. For comparative purposes, we again collected data on recognition performance as well, this time using the yes–no procedure.

Method

The two independent variables of the experiment were both manipulated between subjects. List length was either 10 words or 100 words. Type of retention test was either fragment completion or yes/no recognition. Subjects were shown either a 10-word or a 100-word list, and following a 10-min retention interval, were tested by fragment completion or recognition in the reverse order of study. The retention interval was filled with playing the same video game that had been used in Experiment 5. In the fragment completion conditions, no fragments of unprimed words appeared in the first 10 or the first 100 test positions, in the 10-word and 100-word conditions, respectively. But fragments of 10 unprimed words were included as a final part of the test sequence. Similarly, no distractor items occurred in the first 10 or the first 100 test positions of the recognition test, but 10 distractor items were appended to the end of each test sequence. The target words consisted of 100 medium-difficulty words selected from the Tulving et al. (1982) pool. The 100 words were randomly divided into 10 sublists of 10 words each. Each sublist was used equally frequently as the critical study list in the 10-word condition, and equally frequently in each of the 10 "sublist serial positions" of the 100-word list. Ten additional words were selected from the general pool to serve as sources of unprimed fragments and as lures in the recognition test. These "new" words were identical for every subject tested in the experiment.

A total of 80 undergraduates of the University of Toronto participated in the experiment for remuneration. They were randomly divided into four groups of 20 subjects each.

Subjects were tested individually. They were informed that they would be shown some words and that their memory for these words would be subsequently tested, although the nature of the test was not specified. Subjects in the fragment completion condition were shown 5 fragments of city names to acquaint them with the fragment completion test. All subjects were also given an explanation of and a short practice session with the video game. Then the study list was presented.

Words in the study list were presented on the screen of an Apple IIe computer at the rate of 1 s/word. Following the presentation of the 10 or 100 words, subjects spent 10 min playing the video game. Testing then commenced. In both the fragment completion test and the recognition test, a constant amount of time was given for each test item, regardless of the amount of time that the subject required or preferred for responding. In fragment completion, subjects had 10 s to complete each fragment by speaking it aloud. Instructions were to complete each fragment with any English word, excluding proper nouns and plurals. No mention was made of the preceding study episode. In recognition, they had 5 s to respond to each test item, judging it "old" or "new' and rating their confidence on a 3-point scale, where 1 expressed low confidence and 3 expressed high confidence.

Results and Discussion

We consider the results first from the point of view of list length and then from the point of view of intralist proactive effects.

The mean proportion of primed fragments completed for the 10-word list was .80 (SD = .17); for the 100-word list it was .59 (SD = .12). The difference was significant, $t(38) = 4.40$, $p < .001$. Unprimed fragment completion performance was comparable for the 10-word list (.18) and the 100-word list (.19).

Recognition hit rate was higher for the 10-word list (.73) than for the 100-word list (.61), $t(38) = 2.82$, $p < .01$. The false-alarm rate was higher for the 100-word list (.19) than for the 10-word list (.11). Because the new words were tested only after all the old words had been tested (after either 10 words or 100 words), it is possible that these false alarm rates were influenced by a positive response bias that may have developed in the course of responding to the old test items in the first part of the sequence. If so, the true difference between the two false alarm rates may have been smaller than the one actually obtained. Wheatever the true false positive rates, however, they would not alter the finding that recognition was lower for the 100-word lists than the 10-word lists.

The confidence ratings in recognition confirmed the picture revealed by the hit and false-alarm rates. The mean confidence rating was 2.65 for the 10-word list, and 2.38 for the 100-word list, $t(38) = 3.57$, $p < .001$. Mean confidence ratings for "new" judgments (pooled over false alarms and correct rejections) were 2.25 for the long list and 2.31 for the short list.

These findings provide evidence that list length has a large effect on both fragment completion and recognition. The fragment-completion data complement similar findings from our pilot experiment and Experiment 1, as well as those obtained by Chandler (1983). The recognition data replicate similar findings reported by others not only in recognition (e.g., Bowles & Glanzer, 1983; Gillund & Shiffrin, 1984; Murdock & Anderson, 1975; Strong, 1912) but also in verbal discrimination (Savage & Kanak, 1973).

Let us next consider intralist proactive effects. The relevant data are provided in Table 7 that shows probabilities of fragment completion and recognition, and confidence ratings in recognition, for each of the 10 deciles (blocks of adjacent words) of the 100-word study list, together with the corresponding data for the single decile of the 10-word list. Because the test fragments and words were presented in the reverse order of their appearance in the study list, the study-list order of the deciles shown in Table 7 corresponds to the reverse order of test deciles for the 100-word list. Inspection of the data in Table 7 reveals that fragment completion and recognition show slightly different patterns. In particular, although fragment completion performance shows no systematic change across the 10 deciles, the recognition data show evidence of a primacy effect and some, though weaker, evidence of a recency effect. We consider fragment completion and recognition in turn.

In a statistical evaluation of differences in fragment completion performance across the 10 deciles, an ANOVA showed no effect of decile, $F(9, 171) = 1.154$. Although performance in the decile studied first and tested last was highest (.63), it was not significantly different from the mean of the middle 80 items (.59) by a Scheffé post hoc comparison, $F(2, 38) < 1$.

Table 7
Proportions of Primed Fragments Completed and Proportions of Hits and Mean Confidence Ratings in Yes–No Recognition in List Deciles of 10 Words Arranged by Study Order: Experiment 6

List length	Study-list decile 1	2	3	4	5	6	7	8	9	10	*M*
					Fragment completion						
10											
M	—	—	—	—	—	—	—	—	—	.80	.80
SD										.17	.17
100											
M	.63	.61	.63	.62	.52	.57	.57	.61	.60	.53	.59
SD	.15	.15	.21	.21	.22	.20	.23	.20	.23	.18	.12
					Recognition hits						
10											
M	—	—	—	—	—	—	—	—	—	.73	.73
SD										.16	.16
100											
M	.76	.61	.63	.56	.62	.59	.52	.60	.59	.67	.61
SD	.19	.22	.18	.23	.21	.22	.23	.16	.20	.16	.10
					Confidence ratings						
10	—	—	—	—	—	—	—	—	—	2.65	2.65
100	2.46	2.38	2.37	2.40	2.40	2.40	2.35	2.33	2.31	2.44	2.38

Note. Test order was the reverse of study order.

Thus, there was no evidence of a primacy effect. The lack of a recency effect is obvious: Performance on the decile studied last and tested first (.53) was lower than performance on other deciles from the second half of the study list. Because we found clear recency effects in the pilot experiment and Experiment 1, where the words studied last were tested for fragment completion *immediately* after study, the negative finding in Experiment 6 may be attributable to the 10-min retention interval between study and test, suggesting that recency effects in primed fragment completion are relatively short-lived.

Comparison of fragment completion performance on the last study-list decile from the 100-word list with the performance on the 10-word list provides direct evidence concerning intralist proactive effects, because the two sets of items differed only with respect to the fact that the study of the former was preceded by the study of the other 90 words in the list, whereas the study of the latter was preceded by no other words. The length of the retention interval, the activity in that interval, and the order of testing of individual items in the two sets were identical. Performance was higher for the 10-word list (.80) than on the last study-list decile of the 100-word list (.53), $t(38) = 4.79$, $p < .001$. On the face of it, this difference might be seen as evidence that intralist proactive inhibition did occur in the 100-word list. However, given the fact that the performance was essentially invariant across the deciles in the 100-word list, the difference is more appropriately interpreted as a necessary consequence of the general list-length effect, the suppression of performance on all long-list items, rather than the effect of early parts of the list on the later part. Only if the observed difference had been accompanied by some kind of a progressive decline in the performance across study-list serial positions in the long list, would a conclusion of intralist proactive inhibition have been justified. Because study-list serial positions were confounded with test positions, our findings do not rule out the possibility that intralist proactive inhibition might be found under conditions where such a confounding cannot work against the discovery of such an inhibition effect, as it did in our experiment.

The recognition data summarized in Table 7 show a pattern somewhat different from those of fragment completion. A one-way ANOVA indicated significant differences in the hit rate across the 10 deciles of the 100-word list $F(9, 171) = 2.60$, $p < .01$. The source of this effect seemed to be limited to the first study decile. Although there was a slight recency effect, the hit rate for the last decile (.67) was not reliably greater than the mean of the middle 8 deciles (.59) of the 100-word list, $F(2, 38) = 1.35$, *ns*.

The data in Table 7 provide no evidence for intralist proactive inhibition in recognition: the proportion of words recognized on the short list (.73) was not significantly greater than the proportion recognized of the time-matched last decile of the long list (.67), $t(38) = 1.22$, $p > .10$. In fact, the small difference (.06) between the groups can be attributed to large word-level primacy effects in the short list. When the first two words studied in both sets are disregarded, the hit rate for the remaining eight words was indistinguishable in the two sets: .67 in the short list and .69 in the long list. However, the analysis of mean confidence ratings does indicate some proactive inhibition. Confidence was significantly higher for the short list (2.65) than for the last study decile (2.44) in the long list, $t(38) = 2.24$, $p < .025$.

Proactive inhibition occurring within a list has been found by others: Schulman (1971) and Bowles and Glanzer (1983) have reported that recognition performance on the latter part of a long list is impaired relative to performance on a shorter list. Schulman (1974) further found that performance dropped appreciably across blocks of items when study and test order of items was the same. However, Schultz and Johnson (1982) have shown that recognition of words and statements from prose passages is not affected by the addition of a proactive portion to the passage at study. Thus, it looks as if the effects of within-list proactive material on recognition depend on the type of material, and perhaps, as our results suggest, on the dependent variable. Our hit-rate data contrast, but our confidence ratings agree, with the typical finding of intralist proactive inhibition in recognition of list words.

The primacy effect in recognition, as shown in Table 7, was clear: The hit rate for the decile studied first (and tested last) was significantly higher (.76) than the hit rate across the middle eight deciles (.59), according to a Scheffé analysis, $F(2, 38) = 5.53$, $p < .01$.

It may be of some interest to note that recognition hit rates were practically the same for the 10 words in the short list (.73) and the first 10 items of the long list (.76). This equality came about despite the longer retention interval for the first decile of the 100-word list than that of the 10-word list, a difference of about 9 min, and despite 90 study items and 90 test items interpolated between the study and test of the first decile from the long list. This finding of no intralist retroactive inhibition is in agreement with the findings of Schulman (1974), who observed equivalent levels of two-alternative forced-choice recognition for the first 25 words from 25-, 50-, and 100-word lists, and also equivalent recognition of the second 25 words from lists of 50 and 100 words. It should also be noted, however, that the absence of any intralist retroactive effect in our experiment may be attributable to a possible bias for positive responses engendered by a test list consisting, except for the last 10 items, of study-list words only. This hypothesis of a changing response criterion is consistent with the greater tendency of subjects to respond "yes" to new items at the end of the long list sequence (.19) than at the end of the short list sequence (.11). It is inconsistent with the observation that the primacy effect was restricted to the first decile: the hypothesis would predict a progressive increase in the proportion of hits across test deciles. On balance, it seems reasonable to conclude that there is no intralist retroactive inhibition in recognition.

In summary, Experiment 6 has shown that 10-min retention performance in both primed fragment completion and recognition is substantially affected by list length: Both measures of retention were lower for the 100-word list than for the 10-word list. Moreover, the effect of list length on fragment completion applies to the entire list inasmuch as performance was uniformly lower for all deciles. Although the observed general inhibition could result from equal and opposite effects of proactive and retroactive inhibition, our view is that the list length effect is a direct result of the number of items presented to the subject at study, with retroaction and proaction playing little role. The effect of list length on recognition, on the other hand, seems to include a specific proactive component.

General Discussion

The main findings of this series of experiments can be summarized as follows.

1. Rapid forgetting occurs in primed fragment completion over a short period lasting no more than five min or so (pilot experiment and Experiment 1).

2. Beyond the first few minutes, forgetting in primed fragment completion occurs slowly but steadily. Performance is lower after 1 week than after 1 hour (Experiments 2, 3, and 4) and lower after 23 weeks than after 1 week (Experiment 2).

3. Despite steady forgetting, priming effects in fragment completion can last a long time. In Experiment 2, reliable differences between primed and unprimed words were still detected more than 16 months after a single brief encounter with study-list words.

4. The course of forgetting over a week is not influenced by "reactivating" the whole list through a recognition test of some of the study-list words given immediately before the fragment completion test for other study-list words (Experiment 3).

5. The course of forgetting over a week is not influenced by the physical format of the presentation of study-list words or the nature of encoding activity at study (Experiment 4).

6. Interpolated study and recall of other lists of words does not produce any retroactive interference with the 20-min retention of primed fragment completion, when compared with the effects of interpolated nonverbal activity, although the same manipulation does have a marginal effect on recognition performance (Experiment 5).

7. Increasing the length of the study list has an appreciable deleterious effect on both primed fragment completion (pilot experiment and Experiment 1, and Experiment 6) and recognition performance (Experiment 6), but in the absence of any intralist proactive interference.

We discuss three main points that the experiments make—(a) the rapid initial forgetting, (b) the longevity of priming effects in fragment completion, (c) the absence of retroactive and (intralist) proactive interference effects—and conclude with a note on the "theory" of priming effects in fragment completion.

Although the results of the pilot experiment and Experiment 1 are not conclusive, they do suggest that the initial rate of forgetting in primed fragment completion is rapid. It runs its course in something like 5 min, reaching what in a short temporal perspective appears to be an asymptotic level of performance and what in reality is a very shallow decay function. The rate of initial forgetting in primed fragment completion seems to be little influenced by the length of the study list, and it seems more similar to that in yes–no recognition than to either free (e.g., Murdock, 1962) or paired-associate recall (e.g., Tulving & Arbuckle, 1963).

The fact of the rapid initial forgetting helps to explain why the amount of "forgetting over a week," as recorded in Table 1, was considerably larger in the Light et al. (1986) and the Chandler (1983) experiments than in Tulving et al.'s (1982) experiment: In the two former, the first test was given essentially immediately after study, in the latter it was given after an hour's delay. But it does not help us to understand why Komatsu and Ohta (1984) found little forgetting over a week, even though their first test was given only a minute after study, or why Roediger and Blaxton (1987a, Experiment 2) found sizable forgetting over a week, even though their first test was given 10 to 20 min after study. These discrepancies presumably reflect differences in the detailed conditions of different experiments, conditions whose identification awaits further research.

Perhaps the most interesting finding was the longevity of retention effects in primed fragment completion. Although, as seen in Table 2, the priming effect had shrunk to only .04 after 23 weeks, it was comfortably larger at .09 after 16 months. The fluctuation is best interpreted, for the time being at least, as some sort of nonsystematic error of measurement.

Two contrasts with such long-term retention should be noted. First, although comparable data are not available concerning recall and recognition of words from a *once-presented long list* it seems reasonable to assume that the longevity of retention is greater for primed fragment completion than for the measures of retention of episodic information. Second, the longevity of priming effects in fragment completion is clearly greater, and indeed of a different order of magnitude, than priming effects in word-stem completion that Graf et al. (1984) reported to be completely dissipated in two hours after study. Graf et al. interpreted priming effects in both stem completion and fragment completion in terms of "activation" of existing lexical or semantic structures, suggesting that differences in the duration of the retention in different tasks may depend on factors such as word frequency and number of response alternatives. We are skeptical about the "activation" hypothesis as an explanation of priming effects that last through two Christmases and two New Years.

The absence of retroactive and (intralist) proactive interference effects in primed fragment completion corroborates similar findings in other tasks (e.g., Graf and Schacter, 1987; Jacoby, 1983b). Interference effects have been typically used to account for forgetting in recall and recognition of episodic information, and indeed in this role they have no rival (e.g., Barnes & Underwood, 1959; Bowles & Glanzer, 1983; Crowder, 1976, chap. 8; Postman, 1952, 1971; Underwood, 1957). Because interference does not seem to provide a viable explanation, it seems reasonable to hypothesize, essentially by default, that forgetting in primed fragment completion is a cue-dependent phenomenon (e.g., Tulving, 1974; Tulving & Psotka, 1971). (We say "essentially by default," because the other possible alternative—forgetting as a consequence of "decay" or "disuse"—has been in ill repute with experimental psychologists ever since McGeoch's (1932) scathing criticism of the idea.) A specific version of the cue-dependency hypothesis might be that, owing to the "contextual drift" (Bower, 1972), the specific encoding operations carried out on the test fragment, or specific skills and procedures that the test-fragment engages (e.g., Kolers & Roediger, 1984), differ from those used at initial study, with the resultant mismatch of operations or procedures and the attendant attenuation of performance. Reinstatement of the study context at the time of the test, on this view, should facilitate performance. Contextual facilitation of priming effects has been reported (Graf & Schacter, 1985; Schacter & Graf, 1986a). Although at present the cue-dependency notion is post hoc, it is testable.

The cue-dependency idea, at first blush, seems less readily applicable as an explanation of the inverse relation between list length and primed fragment completion, observed in our Experiment 6 as well as in the experiment by Chandler (1983). But it may turn out to be suitable on further thought and analysis.

For the time being, and pending further study of the issue, we note that the longevity of priming effects in fragment completion stands at least empirically in good harmony with the difficulty of observing retroactive interference effects in the task: Lack of interference effects and longevity of retention would be expected to go hand in hand. If we assumed any similarity at all in the effects of interference produced by laboratory and extralaboratory events, and if we knew that laboratory events are effective in producing sizable amounts of interference over short intervals of time, we could not expect to find the kinds of long-lived aftereffects in primed fragment completion that we in fact did. This line of reasoning leaves open the possibility that retroactive interference effects may occur in primed fragment completion, but they may be revealed only in experiments that are much more sensitive than ours. The relation between list-length effects and the absence of intratrial proactive (as well as retroactive) interference is a separate, vexing, as well as a more general issue. We felt obliged to mention it, although we have no insights into it to offer at this time. The possibility that the reverse testing order creates a situation in which proactive encoding effects are balanced by proactive testing effects (Schulman, 1974) is worth examining before any other action is taken.

Finally, what kinds of theoretical lessons about priming as a form of learning and retention have we learned in this research? Specifically, what can we say about the "nature" of priming, at least as it manifests itself in the task we studied, word-fragment completion? In Tulving et al.'s (1982) article, in which priming in word-fragment completion was shown to be stochastically independent of episodic recognition as well as more resistant to forgetting than recognition, it was tentatively suggested that priming may reflect the operation of a memory system other than that subserving recall and recognition of episodic verbal information. The notion has been further discussed and elaborated elsewhere (Tulving, 1983, 1985). Others have offered different interpretations of priming and priming effects in a variety of tasks (e.g., Blaxton, 1985; Craik, 1983; Graf & Mandler, 1984; Graf et al., 1984; Jacoby, 1983b; Roediger & Blaxton, 1987b; Scarborough et al., 1977; Schacter & Graf, 1986b). This is not the time or place to try to adjudicate all the different ideas concerning priming and priming effects. Suffice it to say that we find nothing in the results that we described here that forces us to reject or revise the idea that priming is subserved by a memory system other than the one that makes possible conscious recollection of events. We also concede, however, that there is nothing in the data that unequivocally impacts on any one of the alternative ideas, with the possible exception of the "activation" hypothesis that we mentioned earlier. The problem of what priming "is," or how it differs or does not differ from other kinds of learning and memory, is still wide open.

Our research has revealed both similarities and differences between priming effects and episodic memory performance. We think that the differences—for instance, those having to do with the longevity of retention effects and with the absence of interference—are particularly interesting, although similarities are also important. Some similarities between measures of memory should always be expected: If different measures of memory had nothing in common, we would have no reason to classify them all as "memory." However, it is the patterns of specific similarities and differences between different memory tasks and memory performance that will help us to understand the nature of priming. The function of experimentation is to identify these patterns.

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